

## ***Interactive comment on “Tropical cyclone genesis across palaeoclimates” by J. H. Koh and C. M. Brierley***

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The manuscript presents an analysis of changes in a tropical cyclone genesis potential index (GPI) for several GCMs in four perturbation climate states—spanning past and future climate. As the authors note, genesis indices have been examined for future climate in a number of studies, but only the recent publications by Rob Korty et al. have examined them for past climates. The manuscript adds to the existing literature by considering simulations of the Pliocene climate and repeating the Korty-led analysis of the climate of the Last Glacial Maximum and mid-Holocene in the newer paleoclimate model intercomparison phase 3 (PMIP3) simulations. Given this contribution, the manuscript should be published following some revisions that largely pertain to the presentation of the results.

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I note that I have not read the other comments before preparing my review.

Major suggestions/concerns:

1) A key concern I have regarding the presentation of the results of the analysis is that the changes in the GCMs' GPI is often discussed as a "response in TC genesis" (e.g., p. 184 L25). It is very important to use clear terminology, as there is research about TC-climate changes that assesses the environmental factors known to affect genesis in today's climate (as encapsulated by genesis indices or GPIs, which is what the authors do) and there is research that uses high-resolution GCM simulations that explicitly simulate TCs (various GCM groups over the last decade, several of which are referenced in the manuscript) with the downscaling approach used by Emanuel falling in the category of explicitly simulating TCs. This is a concern for both clarity for readers and accuracy—the ~300 km coupled GCM simulations analyzed in the manuscript cannot tell us how "TC genesis changes" but can tell us how "environmental factors that affect TC genesis change" or "how GPI changes". This issue comes up in the main text, Table 2, and most importantly the title of the manuscript. I suggest the title is changed to something along the lines of "Tropical cyclone genesis indices across palaeoclimates".

2) I think it is important to note that GPIs have difficulty when climate changes are separated into direct responses to forcing (e.g., change in CO<sub>2</sub> concentration) and temperature-dependent responses. Held & Zhao 2011 (and a couple of early references that you can find therein) show there are direct TC frequency changes from forcing in TC-permitting GCM simulations and Camargo and co-authors have tried various GPIs on this case ("Testing... HiRAM..." J. Climate).

3) Using the same 'b', the coefficient that allows the index to match the observed amount of genesis, for all the simulations is problematic when the ensemble mean is the focus. Table 2 shows that this results in factor of 3 inter-model variations in the GPI's

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global number per year. An example: if you have one GCM with 90/yr (like CCSM) and one with 30/yr (like IPSL) and they have opposing 10% changes, the ensemble mean would have a ~5% change. Whereas, if 'b' was adjusted so that all models had the same preindustrial N/yr, opposing 10% changes would lead to no change in the ensemble mean.

4) The manuscript does not have too much discussion or figures of how individual factors in GPI change across the climate states. This is one of the appealing aspects of using GPIs—they can be broken down to say which environmental factor dominates the change in the index. It might be nice to include more summary of this, but I understand that it would be difficult to systematically present all of the factors for all of the climate states.

5) In 4.6, I think it would be nice to also express the change in global GPI in % per K of tropical SST change. It doesn't look like one number will work across climate states and this is an interesting result.

Minor suggestions/concerns:

\* Stating the parameters used for the potential intensity calculation is good to ensure reproducibility. Also, It's conceivable that this accounts for some of the divergence between the manuscript's assessment of the RCP scenario and the other publications that have assessed those same simulations.

\* There is some discussion of the role of the ITCZ on TC genesis (abstract and section 5). I suggest the authors check out my 2013 GRL paper co-authored by M. Zhao and I. M. Held and a JAS paper that is in press led by Andrew Ballinger (with 3 other authors from the GRL) for research that is directly examining this connection.

\* Introduction the GPI used in this paper. I think the lineage of the index used in Korty et al. is a combination of Tippett et al. 2010 and Emanuel et al. 2010, which in turn modified the Emanuel and Nolan 2004 index. Emanuel et al. 2010 increased the

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exponents on the vorticity and shear over EN04.

\* In 4.6, the text has a different number of the Pliocene compared to figures 9 and 10.

\* In section 5, Bengtsson et al. 2006 and Held and Zhao 2011 have also connected strength of convection to TC genesis. I have a personal interest in knowing if the ascent over the ocean weakens in the mid-Holocene simulations, as I found this in idealized simulations of orbital precession (Merlis et al. 2013 Part I, J Climate). I understand this is not central to the manuscript.

\* It would be interesting to know what the GPI is for the fixed SST Pliocene simulation used in Fedorov et al. 2010.

\* Angular precession in Table 1 should specify that the angle is relative to the NH autumnal equinox.

\* For the variability of the genesis index: I think this is a lower bound on the variability of TC genesis. Villarini and Vecchi have a series of papers using statistical estimates of TC activity based on environmental factors, so you could see what their interpretation is of this.

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